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CONTRIBUTIONS TO THE LIFE-HISTORY OF PLANTS. No. XIII.

BY THOMAS MEEHAN.

I. Sex in flowers.—Corylus rostrata.

Since the publication of my first paper on the law governing the sexual characters of flowers,[1] and others subsequently on the same topic, the evidence of the soundness of the principles therein presented is everywhere so palpable that I have for some years past given up noting it and placing it on record. The principle then and subsequently made plain is, that in the earlier stages of its life a flower bud may be either male or female, and that the final determination of sex is a mere question of nutrition. If there be an abundant supply of nutrition available, or vital power capable of availing itself of the nutrition provided, the female characteristics prevail. With lessened nutrition, or of vital power in the floral matrix to make use of nutrition, the male characteristics result. These views, opposed as they were by eminent biologists, and even ridiculed by my esteemed friend Prof. Agassiz on the reading of my paper, I have lived to note are generally accepted, though at times it seems to me that a few more recorded observations might be profitable. Before me at this time is an able paper by Prof. Kenjiro Fujii, of the Imperial University of Tokyo, Japan, on sexuality in the flowers of *Pinus densiflora.*[2] He finds in his observations that the sex of the flowers is undetermined until a certain stage of development, and that a flower which would otherwise develop into a male has a tendency to become female when local increase of nourishment takes place at a certain stage, or during certain stages of its development. This seems so like the language of my papers of a quarter of a century ago, that it is pleasant to know that Prof. Fujii has worked the conclusion out in utter ignorance of my having occupied the field before him.

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[1] Proceedings of American Association for the Advancement of Science, 1869, p. 256.

[2] Tokyo Botanical Magazine, Vol. ix, No. 101.

I will give a striking illustration of the soundness of this principle, which I have not hitherto recorded, in *Corylus rostrata.*

In one of my earlier papers[3] I recorded that there was a uniform line of character dividing the species of western Europe from their close allies in eastern America. Among other points, the tendency to lateral branching is more marked in the European than in the American line of the same family. *Corylus Avellana* and *Corylus rostrata,* which I had not in that list, are good illustrations. In a cubic yard of growth there would seem to be hundreds of twiggy branchlets in *Corylus Avellana,* the common European Hazelnut, to the scores only in the North American Hazelnut, *Corylus rostrata.* It is important to note this difference in these species, in connection with these sexual questions, because at first glance the male catkins seem terminal in the European, and axillary in the American. The male catkins seem situated on comparatively strong branchlets; while the female buds seem to be on branchlets often weaker, and situated below the males and more unfavorably located as regards nutrition. That this is deceptive will hereafter be shown.

The branching of *Corylus rostrata* is remarkable for its stiff rigid character. It might be taken for some willow at first sight, rather than a relative of *Corylus Avellana;* although this latter species has this culm-like or reedy appearance before the plant reaches its flowering stage. The flowers appear along the whole length of these rigid branchless stems. The male flowers are produced from the lower nodes, and the female from the upper ones. ***But the proportion of each on the separate branches is in exact proportion to the vigor of the branches.*** On a very slender twig of perhaps twelve or fifteen nodes, there will be a male catkin from every bud. On the branches of medium strength the proportion of male to female will be equal, while on the branches of superior vigor all but three or four of the lower ones will be female. On a strong leading shoot before me, and which is characteristic of numbers of others on the plant from which it was taken, the four lower buds only have catkins; while the ten upper buds have either female flowers or a few strong buds to make branchlets for next season.

No one closely observing a vigorous plant of *Corylus rostrata* can fail to be impressed with the truth of the principle I have advanced, that the question of sex in flowers is merely a question of nutrition in an early stage.

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[3] "On the Relative Characters of Allied Species of European and American Trees" (Proc. Acad. Nat. Sci. Phila., 1862, p. 10).

For reasons already given, the fact is not so apparent in *Corylus Avellana.* In the later period of the growing season, in this species, some of the axillary buds develop to short branchlets instead of remaining as buds to bear flowers, and make growth for the coming year. It is from the axillary buds of this secondary growth that the male catkins appear, and thus present the appearance, from their elevated position, of being superiorly situated as regards nutrition than the female flower buds, which are only apparently below. When closely examined it will readily be seen on how weak the axes and how very unfavorably for nutrition these buds of the male catkins are situated.

It may be noted that the axial buds, bearing the male flowers in *Corylus rostrata,* develop slightly to a branchlet in the fall of the year, but I have never seen one to reach more than a quarter of an inch in length.

I know of no monœcious plant that does not show a more or less continuous activity during the early autumn, or even during the winter season if a few days of springlike weather occur, and the postulate may be presented that a marked characteristic in the growth of a female flower is the greater conservation of energy during its early stages, as compared with a tendency to reckless expenditure in the male flowers. After all, this is but another way of expressing what has already been made plain, that the ability to store and to profit by nutrition accounts for sexual differentiation.

II. CLETHRA ALNIFOLIA IN RELATION TO ITS MORPHOLOGY.

In families where there is considerable irregularity in the number and arrangement of related plants, it is important in what might be termed genealogical botany that the primary type should be well understood. In Ericaceæ, for instance, we have genera with varying numbers in the calyx, corolla, stamens, and styles; some are gamopetalous and some polypetalous, while some have inferior and others superior ovaries.

As all parts of a plant from the starting in growth of the seed to the final maturity of the flowers it produces are but modified leaf-blades, the arrangement of the leaf-blades on the primary stem should furnish a good foundation for subsequent investigation. Taking a fairly vigorous branch of *Clethra alnifolia*, we note that the phyllotaxis is on the $\frac{2}{5}$ plan. We may, on this fact, look for a pentamerous system to prevail in the subsequent development, and charge to consolidation or to abortion any variation from this type. Following again the *Clethra*, we find a five-cleft calyx, and we say it is normal. Then we have five petals alternate with the sepals, which again we pronounce normal. But the next cycle appears as ten stamens, which for a single cycle would be out of order. Observing closely, however, we note that five of the ten are longer than the others, and we learn that there are really two cycles of five, of which the long stamens represent the outer.

Taking up now the gynœcium: the stigma is noted as being slightly three-cleft, indicating a tricarpellary ovarium. There should be five. Two are wanting. This sudden departure from what has hitherto been a regular pentamerous series, suggests that there has been a suppression of several verticils since the appearance of the last staminal whorl. A glandular disk fills the interspace, and we may reasonably conclude that herein lie the missing links. The carpellary system here is parallel to prolificacy as often seen in flowers. One rose proceeds from the centre of another rose, or a small orange at the apex of a larger one, as in the variety known as the navel. The upper one, as in the case of the gynœcium of *Clethra*, loses much of the normal vitality of the individual.

In some genera of Ericaceæ cohesion—vital power in its higher stage—plays a part in organizing, the reverse of the lower degree, but still with the same result in the disappearance of parts that me may term primarily separate. In examining a large number of *Clethra* flowers, a pair of stamens will occasionally be found as completely consolidated into a single filament as the carpels have had the three styles united· the extreme upper portions of each, however, continuing normal. It shows a tendency to consolidation. From a consideration of all the facts, we may conclude that the primary type of Ericaceæ is pentamerous and polypetalous, that those approaching this type are the oldest in the genealogical line, and that all the variations from this type in the order are deducible from the variations in growth-energy resulting in the abortion or consolidation of primary parts.

III. SANICULA—A BIOLOGICAL STUDY.

Examining a plant of *Sanicula Marilandica*, a striking point presents itself in its branching character. These branchlets are usually in threes, and exceptionally in fours. That acute observer, Rafinesque, noticed this, and it was doubtless on account of this striking character that he proposed the name of *Triclinium*, when he thought to make a new genus out of the older *Sanicula*.

I have endeavored to teach in numerous papers, that by far too much is claimed for environment in the evolution of form. It would rather seem that form depends on a purely mathematical law of growth-force. Growth is not a continuous operation, but rhythmic. The degree of force depends on a supply of nutrition and the ability of the plant cell to avail itself of the supply. As, therefore, each rhythmic wave varies in intensity, the ultimate form of the immediate structure under the influence of that wave must vary accordingly.

Again I have taught that even sex is influenced by like conditions. Those parts of the flower or portions of a plant under the influence of a vigorous growth-wave laden with nutrition favor the production of female organs—the feeble wave is productive of male organs only.

A study of *Sanicula* well illustrates these points. The first growth-wave ceases at the first pair of leaves. This must have been very sudden; for the main axis has been so deprived of food, and its vital power rendered so weak, that it can only make a feeble growth with the advance of the next wave. The consequence is that the growth-wave is turned into the two axillary buds. They take the place of the original axis, and we have two leading branches instead of one. The original axis remains a weak, threadlike, common pedicel, which bears usually but a single fertile flower and numerous barren ones. In some species of plants the central axis utterly fails under the shock of the rhythmic growth. In that case we have the dichotomous mode of branching.

Passing the eye along the stem, we find the growth-wave, rhythmic as it is, decreasing in vigor. The central axis profits more by nutrition—the branches from the axial buds are less like leaders—and by the time the terminal point is reached we find it surmounted by a fruit on a pedicel much stronger than the lateral ones.

There are but three lateral branchlets, though occasionally four; and the retarding of the axial growth has been of such a severe character, that the axillary origin of the lateral branches has been totally obscured. The pedicillate male flowers originate from numerous axillary buds on these fruit-bearing branchlets, the external evidence of such origin being also obscured. It is believed that a careful study as indicated will be convincing that it is the degree of the rhythmic growth-force that decides the character of the plant, and also the relation of the sexes to each other.

Now let us take *Sanicula Canadensis.* With the light already gained, we see that nothing but a variation in the force of the rhythmic wave has made a distinct species. The characters are all of the same class, but there has been acceleration and retardation of the wave force in different directions. The axillary branchlets have continued to retain the attention of the growth-wave to the last, and now bear the female flowers—while the weaker central one has accorded to it male ones only. Examining, now, the Old World species, *Sanicula Europœa*, we see scarcely any difference except such as might occur from variation in the degree and direction of the growth-wave. Nutrition has been diverted from the central axis to a greater degree than even in our own *S. Canadensis.* It produces only short-stalked male flowers.

We here have a genus in which we can clearly see how the species were made. The degree of force exerted by the rhythmic wave started the process, and the assimilation of nutrition completed the task.

The study brings us nearer to a perception of the origin of species. But we may not yet perceive the hand that set the process in motion. The characters that distinguish these three species seem permanent now. They do not change under any conditions of environment existing at the present time, however varied they may be. Though we may say that the three species would easily be produced out of one type, simply by varying degrees of growth-force; the conditions under which this variation occurred, and the period in the world's history when they occurred and became hereditary, is still a mystery.

IV. ROSA RUGOSA IN CONNECTION WITH THE EVOLUTION OF FORM.

A few years ago I crossed a flower of *Rosa rugosa* with pollen from the well-known hybrid perpetual, General Jacqueminot. It may be noted here that in this locality the rose anthers mature almost simultaneously with the expansion of the petals. To be certain that a flower will have no aid from its own pollen, my practice is to open the petals carefully the day before they would expand naturally, remove the stamens with the unopened anthers, and then at once apply the foreign pollen. Though I believe pollen will remain potent at the apex of a stigma for an indefinite time, so as to be ready to perform its function the moment the stigma becomes receptive, I apply more pollen after the petals open normally. With these precautions I feel safe in my experiments against the interference of undesirable pollen. These precautions were taken in this experiment with *Rosa rugosa.*

In the autumn the seeds were cleaned from the capsule and sown in the open ground.

The following summer two plants only had made their appearance, and attracted attention at once from the fact that one of them had the appearance in foliage and habit of General Jacqueminot, and the other somewhat after the manner of *Rosa rugosa*, the female parent. The first year of seedling roses results in comparatively feeble growth. The stronger growth of the second year was eagerly looked forward to. When that time arrived the one resembling the male parent was stricken with a fungous parasite, and so severely that, in spite of our efforts with copper solutions, the branches were no stronger than they were the year before. The vital power was so much reduced that it was totally destroyed by the subsequent winter. In spite of its weakened condition, it retained to the last its striking resemblance to the hybrid perpetuals of which General Jacqueminot is a type. The other flowered, and to my great surprise was not *Rosa rugosa*, but a perfectly typical specimen of *Rosa cinnamomea!* It is still flowering at this date, June, 1897, and is simply the cinnamon rose.

To carry this curious experiment further, I had a small quantity of seed of *Rosa cinnamomea* and *Rosa rugosa* sown near to each other where comparisons could be made as they grew. There were something less than a hundred plants in each lot. Both sets were nearly identical in character as they grew. The branches were slender, smooth, with scattered spines, and no aciculæ. As the plants gained vigor, strong branches, densely clothed with bristles, would appear from the *rugosa* plants, and ultimately became absolutely that species. Some of them would, however, continue to produce some smooth slender branches; and these, flowering, were purely *Rosa cinnamomea*—the two species flowering on the same plant. These separate blocks of roses are still blooming where they were sown, and in the bed of *Rosa rugosa* there are ten plants that still continue *Rosa cinnamomea.*

The bed of *Rosa cinnamomea* continued its comparatively weak growth characteristic of the species. Occasionally a strong shoot would push out from near the ground covered with bristles as in *R. rugosa;* but it would eventually weaken, become smooth, and have the smaller flowers, foliage, and, in fact, be nothing but *R. cinnamomea.* One only assumed a more vigorous and bristly character. This plant has the flowers as large as those of *R. rugosa*—that is to say, they are more than double the size of the normal cinnamon rose, though continuing the pale rose color instead of the deep crimson of *R. rugosa.* The foliage also, though not as coarsely wrinkled, that is to say, rugose, as the other, still is diverse from the type, and is an advance in the direction of the latter rose.

Two remarkable facts are made clear by these observations:

First.—Two totally distinct forms can be produced from the same seed vessel, though every condition surrounding their inception seems exactly the same.

Second.—*Rosa rugosa,* a species with characters markedly distinct from *Rosa cinnamomea,* is nevertheless an evolution from that species, and has been produced by the energy of a more vigorous development.

We may conclude from these facts that however much may be conceded to gradual modifications extending over indefinite periods as an agency in the evolution of species, development need not be dependent on this principle alone. The varying degrees of internal energy must be credited with producing sudden and remarkable changes, though we may not yet be able to perceive clearly the nature of the motive power that induces it.

V. VIOLA IN RELATION TO POLLINIZATION AND FECUNDATION.

Though it has been shown by many close observers that the general opinion is erroneous that early or perfect flowers of many species of *Viola* are infertile, the impression is still widely prevalent, especially in the case of the very common *Viola cucullata.* I have myself held that opinion, though I have placed on record that they were abundantly fertile when growing on a dry rocky piece of ground. I had, however, never made the continuous daily observations on them that I have made on other plants, and so made it a point to do so, commencing the task on the first of April, 1897.

I have an orchard of about half an acre, partially shaded by the branches of the apple trees meeting together. The surface was originally in grass, but at the date given only a few tufts remained. The whole at the date named was a sheet of violets in bloom. These many thousands of plants all sprung from a few. I noticed these a quarter of a century ago. Myriads of seeds from last year's crop were showing cotyledons. By sheer force of numbers they were crowding out all other vegetation, and it is a question whether their operations had not as much to do with the destruction of the original grass as the shade which the apple trees afforded.

Though from a few original plants, the assemblage at this date presented a remarkable variation in the form and color of the flowers. Some had petals almost linear, others petals almost round. In some cases the two upper petals would be nearly round, and the three lower ones very narrow. The color of the petals varied in innumerable shades, from the normal violet to nearly white in one direction, and to nearly red in the other. Some plants would have the peduncles little longer than the leaves; others would have the flowers on long and slender peduncles. A notable fact, however, was, that with all this variation in the flowers, not a trace of variation could be found in the plants themselves. In foliage and habit, every plant seemed an exact repetition of another one. As conditions of environment could have nothing to do with these variations, and as there were no varieties to aid variation through cross pollination, there can be but one deduction, that internal energy alone is responsible for the changes.

My next thought was to watch for insect visitors. Winged insects are scarce with us in April. I saw none among these plants during the whole month. I cannot say that these millions of violet flowers had no insect visitors during that period, but it is safe to say that if any were visited by insects the number would be so inconsiderable as to have no influence on the result.

Numberless flowers were opened from day to day. All the anther-sacs were abundantly supplied with perfect pollen. From the earliest period the upper portion of the style, with its stigma, would be in advance of the stamens. When the pollen would be projected from the anther-cells, the membranous appendages of the anthers would be so closely appressed to the style that it would be very difficult for pollen to get through to reach the stigma. Though pollen grains could get through this close-fitting sheath, the stigma is so thoroughly encased by the mass of hair in the throat, that it is inconceivable that pollen should ever reach it. As a matter of fact, I could never find a grain of pollen on the portion of the style above the membranous sheath, and of course none in the stigma, though the ovarium clasped by the anthers themselves would be completely pollen-dusted.

I was satisfied from these observations that the flowers could not prove fertile, and was prepared to believe that so far as *Viola cucullata* is concerned, the popular impression regarding infertilty in this species was correct. I started to examine several other species in the same relations. *Viola tricolor* is conceded to have its early perfect flowers fertile. I examined some flowers from the wild species growing in my garden. I found the stigma barely extending beyond the membrane sheath, which, by the way, was not closely clasping, nor was there a mass of hair on the petals to guard the receptive portion from an attack by the pollen grains. It did seem that the fecundity of the early flowers could be accounted for. But again I could not find a single pollen grain on the apex of the style, or stigma, as we would terminologically say!

By the time the first week in May was reached, I was surprised to find an abundant crop of seed vessels on the earlier flowers in the orchard, the peduncles curving toward the ground just as they do in the cleistogamic condition. A number, probably one-third, had failed to perfect seed, but they had advanced considerably before withering, showing that their early demise was not from non-pollination, but from a failure of nutrition, just as in many fruit trees we find the ground beneath their branches strewed with immature fruit, which has fallen, after being fairly fertilized, by the failure in the stock of nutrition to bring them to perfection. In the carefully noted observations of Mr. Darwin and others in the Old World, on the fertilization of flowers, failure of nutrition is rarely permitted to account for the failures to seed in many of the experiments noted.

I have for some time past concluded that, in some manner not yet demonstrated, pollen tubes may and do at times reach the ovules otherwise than by way of what we commonly know as the receptive portions of the stigma. In *Viola tricolor* this may occur by way of the filmy hair on the dorsal part of the stigma, which the pollen may reach in this species as already noted, and the question may arise as to what is the stigma proper in *Viola.* It is customary to regard the small opening at the apex of the thickened style as the stigma. I am inclined to regard it rather as a nectariferous gland. In examining flowers of *Viola cucullata,* just before they expand, the orifice of this tube is closed. When the flower is examined the day following the opening, a globule of very sweet nectar occupies the position; the following day this disappears, and the hollow cavity noticed by various authors is evident under a good lens. This open passage is hardly consistent with the general character of a passage way for a pollen tube which has to depend for material in building up its structure on the cellular matter which it meets with on its journey to the ovule. Nor can I recall any instance in which such a large proportion of nectar is secreted by the receptive point of a stigma. On the other hand, if we believe, as we undoubtedly may, that nectariferous glands are atrophied primary structures, it will be difficult to trace the morphology of such a gland situated in the position this occupies.

But the morphology of the violet presents some anomalies. Though it is certain, as I have demonstrated in various papers, that the leaf does not always originate from the node from which it seems to spring, and that it is the union of the edges of the leaf-blade that causes what we know as decurrence in stem structure—it does not follow that decurrence is not sometimes really *de*-currence. In many species of *Lactuca* from the south of Europe and eastern Asia, this is evidently so; and in *Viola* the auricles at the base of the sepals would undoubtedly have to be accounted for in any fair conception of the consolidated parts of the flower. I take it we are to look in this direction for the origin of the spur in the petals of the violet, and in the fleshy appendages to the stamens, sometimes called nectariferous glands, which fill the spur spaces. Mr. Darwin observes that he once saw *Bombus terrestris* slit the spur of *Viola canina* in search of the nectar these glands secrete, and it is generally supposed they are nectariferous. But certainly in *Viola cucullata* they excrete nothing, as I infer, from not having been sufficiently differentiated from their primary condition to be perfect glands. Taking now, as we may again, the consolidated style, as not merely the elongated extension of the carpels, but also of the primary axis as well; and the thickened upper portion of the style as a feeble effort to form another verticil of floral organs, the apex of the primary shoot would be extremely likely to end in a complete gland. If this view be correct, we have to look elsewhere for the passageway to the ovules; unless indeed we conclude that it is possible for nectariferous glands to serve as ducts for pollen tubes in some instances, a circumstance I have sometimes suspected.

VI. ISNARDIA PALUSTRIS—ADDITIONAL NOTE ON ITS STIPULAR GLANDS.[4]

I noted in 1886 that the usual description of Onagraceæ—leaves without stipules—was not strictly correct, as they were present in the form of glands in *Isnardia* (*Ludwigia*) *palustris*, the marsh purslane. Dr. Asa Gray, to whose regular and friendly correspondence through a number of years I am greatly indebted, wrote: "You have certainly found something; but whether these glands should be considered stipular or not, I am not prepared to say." The matter had passed from my mind till recently, when the opportunity occurred to spend several hours in the midst of a large quantity of the plant.

At the base of the solitary axillary flower is a pair of minute scales. The general resemblance of the gland and its connection with the base of the petiole suggested the possibility that these glands might also be bracts or scales that had come within the connate power so potent in this species. This power is well exemplified in the structure of the flower. The stamens are opposite the sepals, indicating that the petals have disappeared. Then we note that where there should have been petals alternate with the sepals and stamens, there are four greenish umbonate glands. These of themselves suggest abortive petals. Holding the flowers up to the light, however, we find by the aid of the lens that the petals are really there, but have been completely covered by the connate sepals, the glands being the thickened apices! May not the supposed stipular gland, in like manner, be some foliar organ almost covered by the union of superior parts? In aid, farther, of this suggestion, we note that the primary origin of the leaf is far below the point from which the leaf proper or blade seems to spring. The consolidated petiole can be traced far below the base of the expanded portion. Each consolidated petiole, however, grasps only one-fourth of the circumference of the stem, thus giving it a sub-quadrangular character. Why might not the supposed stipule be the apex of another pair of minute scaly bracts, all of which but the extreme points had been covered, as the petals were, by the superior size of the consolidating parts above? With these suggestions we examine the stipular glands closely, only to find that they are opposite to, and not alternate with, the pair of scaly bracts above them. We must still believe they are stipular appendages of the leaves, and not the upper remains of independent but submerged organs.

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[4] Proc. Acad. Nat. Sci., 1886, p. 349.

Of late years it has been found that what appear as striking exceptions to general rule can be accounted for in the regular way, the operation of the law only having been obscured. Why *Isnardia* should yet seem a striking exception to the absence of stipules in Onagrads still remains a problem.

During the examination of these plants the absolute fertility of the flowers was suggestive of perfect self-fertilization. Few axils but had their primary branching system retarded for the production of a flower instead. Not a solitary imperfect seed-vessel could be found, out of the many hundreds on a single branching plant. Self-fertilization was found to be the fact. Not only do the anthers discharge the pollen simultaneously with the expansion of the sepals; but they rest on the stigma, so that it is impossible for any external agency to interfere with their work.

Another interesting observation is that at the expansion of the sepals, nectar copiously exudes from the umbones above noted. No winged insect appears to visit these minute flowers.

It would seem from these observations that the gland in *Isnardia palustris* is really stipular; that the petals are not abortive, but have been covered by the connation of the sepals; that the flowers are arranged so that self-fertilization must ensue; that these self-fertilized flowers are enormously productive; and that the production of nectar, so far as the visits of insects may be concerned, is superfluous.

VII. PARTHENOGENESIS.

It is about two hundred years ago since Camerarius recorded the fact that female mulberries and other trees would produce fruit without pollinization, though such fruit was sterile. These observations have since been abundantly confirmed. The necessity of pollen to fertile seed came to be regarded as absolute law, until some fifty years ago, when the Curator at Kew, Mr. John Smith, announced that an Australian plant, of which he had but one female specimen, perfected its seeds. It proved to be a new Euphorbiaceæ, and he named it *Cœlebogyne ilicifolia*—the generic name from its supposed parthenogenetal character.

The author of this paper was a student in Kew at that time, and well remembers the incredulity with which the announcement was received, that nature should seem to make a universal law in relation to method of reproduction, and yet make a striking exception in this case. Nature furnishes infinite variation, but these variations seem to be only of one general plan. It seemed more probable that, in some method unexplained, pollen had been formed, and really pollinated the embryo. It does not appear that any further observations on this plant were made at Kew, or, if made, recorded.

Strasburger took up the subject again in 1878, but though my good friend, Mr. George Nicholson, Curator of Kew, writes under date of April 10, 1897, that "the whole business has been threshed out by Strasburger," the latter seems to be more concerned about the cell development than the manner of its fertilization. This is especially true of that part devoted to the Castor-oil plant, *Ricinus communis*. In *Cœlebogyne*, he insists that the true embryo does not develop, but that the seeds proceed from adventitious buds from the wall of the ovary. One may conceive of them as bulblets, analogous to what we find in viviparous flowers; but he does not explain how cotyledonous seeds are constructed in this way.

In 1894 the author of this paper decided to make for himself careful observations on *Ricinus*. A number of plants were set out in a mass where he could easily watch them. Every male bud that could be observed was rubbed away before expansion. For all this an abundance of seed matured. It was found subsequently that there were other plants within a quarter of a mile. It was thought best to repeat the observations another year. The following year no plants were near but those under observation, and it began to look as if the plants had no use for pollen. Still there seemed doubt, and the observations were continued yearly to the end of 1897—all with the same result. Dr. Ida A. Keller, Professor of Biology in the Philadelphia Girls' High School, an experienced microscopist, aided me considerably in the observations made, and only for the fact that in one of her acute examinations she discovered a pollen tube at work on the ovarium, there would have seemed no room for doubt that pollen was unnecessary for the production of perfect seed in *Ricinus*. It was determined to try once more. The past year, 1898, only one plant was allowed to grow, and only one flower spike permitted to remain. All the others were cut away in the bud. This permitted closer examination, which was made almost daily. I noted for the first time that there were on the pedicels with the female flowers, buds scarcely larger than pin-heads, and, under a strong lens, evidently antheriferous. These were carefully cut away. As the female flowers opened they all withered, unlike their fertility in former years. There seemed no doubt that pollen was essential. After half the spike had fallen away, the upper portion, say some twenty flowers, showed a disposition to permanence. The pretty fringed pistils protruded and continued perfect for many days. Finally, the capsules enlarged until they had about reached the usual dimensions. Now it seemed as if the plant would produce seed without the aid of pollen. After turning brown and seemingly ripe they were found to be as empty as Camerarius' mulberries were.

While under the impression during the earlier years of these observations that *Ricinus* was fruiting in the absence of pollen, I was encouraged by observations seemingly confirmatory by Judge Day, of Buffalo, and Prof. Greene, of Washington, on other plants. At the same time I had noticed that solitary female plants of *Gingko biloba* were fruiting abundantly in Germantown, while the only known male plant was at Woodlands, some ten miles away. Recently Japan botanists have discovered spiral coils of spermatozoids in the generative cells of *Gingko,* and others have observed them in *Cycas revoluta.*

The conclusion reached by the author is that though for several years he regarded the Castor-oil plant, *Ricinus communis,* as a genuine case of parthenogenesis, the past year's experience still leaves the matter open to doubt. If it be true that the female flowers of *Cycas revoluta* can generate spermatozoids in their ovaries, and thus self-fertilize the ova, the occurrence must be rare. In this vicinity old specimens of this plant are frequently seed bearing, *apparently,* but in every case examined by the author they were found to have only empty capsules.

It seems to the author that the subject of parthenogenesis is by no means thoroughly "thrashed out," and the object of this paper is to encourage continued observations.

VIII. LACTUCA SCARIOLA, IN RELATION TO VARIATION AND THE VERTICAL POSITION OF ITS LEAVES.

Lactuca Scariola is spreading rapidly over the United States. Some twenty years ago I was led to my first acquaintance with it through the kindness of Dr. George Engelmann. It was then growing in the vicinity of St. Louis. It interested us from the fact that we thought we saw polarity in the leaves. The plane of the leaf was vertical, and it seemed that the edges were directed due north and south. The plants were growing thickly together, and in the cases which did not come under this rule we concluded that the crowding interfered with the natural tendency. Three years ago, 1894, a solitary plant appeared in my garden. The behavior of a species is always instructive when we can watch its distribution and development from a single plant within a limited area. Being an annual, and flowering after the garden weeding has been completed, it is a comparatively harmless weed. I allowed the plant to seed, and the seeds to be distributed by the wind. The next season a number of plants were allowed to seed. This year, 1897, there are hundreds of plants growing within an area of twenty acres. Many of these I am leaving to grow till they reach the flowering stage, when they will be destroyed.

The variations from the one original plant, and all under the same conditions of environment, are very remarkable. In some the leaves are about four inches long by three wide ; that is to say, broadly ovate, while there are some with leaves nine inches long, and from one to two inches wide. In these plants the long linear leaves are obtuse. In others the leaves are some nine inches long, but widest in the middle—these are sharply acute. The leaves are usually entire, but there are a number that are lyrately pinnatifid. In one case the lobing is so deep that the divisions are little more than narrowly winged nerves. In most cases the leaves are flat at the margins, but in a number of cases they are strongly undulate. One of the most striking variations is in the habit of the plant. In the majority of cases the main stem is less than half an inch thick at the base, and grows up rapidly with wide internodes; in others, the stem will be nearly an inch thick, with little disposition to elongate; the lateral buds develop to branchlets, and the plant becomes densely bushy, with scarcely any internodes. In these cases the leaves are cuneate at the base. The only characters that seem uniform through this extensive range of variation is the glaucous green of the foliage, the vertical twist to the leaves, and the ridge of bristles along the midrib on its under surface. Surely we may conclude that the variations presented in this case are due to the degree and direction of life energy, and that the common phrases of " cultivation " and " conditions of environment" have no meaning here.

The vertical plane of the leaves next commands attention. A careful examination of a number of isolated plants soon leads to the conclusion that there is no polarity—as it is generally expressed. The plane of the leaf is in any direction. There are no more in a northward than in a westward line. I am at a loss to imagine how I, in common with other observers, ever believed it could be classed as a " compass plant," unless it be on the general principle that it is easy to see what we are looking for.

In the examination a novel point presented itself. The leaves are vertical instead of horizontal by reason of a twist just above the point of divergence from the stem. But on every plant there are cases of twists in opposite directions. In former " contributions " I have shown that antidromy is common. There could not be secund inflorescence without it. Two or three leaves in this

Lactuca following regularly the spiral against the sun would have the leaves back to back, when the next would twist in the opposite direction so as to face the lower leaf. The occasional pairs of leaves, drawn together in parallel lines, present a very novel and interesting appearance. These cases of antidromy follow no numerical rule. The most striking cases of parallelism will naturally be those where two successive leaves in the spiral face each other; but generally it is the next in the series, or, in some cases, the third or fourth leaf that turns its back on what seems to be the normal spiral course.

It may be here noted that the numerical order in the phyllotaxis is not as definite as it is supposed to be, and this fact will have some bearing on the phenomena connected with *Lactuca Scariola.* In many plants which have a normal $\frac{2}{5}$ arrangement, $\frac{1}{3}$ cases are not infrequently on the same stem. This may be well observed in the thick-stemmed Centaureas.[5] The upper portion of the stem thickens from a considerable distance below the flower, and the numerical arrangement changes according to the degree of thickening. The truth is, as laid down already in these contributions, the cortical layer in a growing branch is made up of the thickened bases of leaves, and the elongated growth of a branch is not in a straight line, but is simply the uncoiling of a growing mass of leaf stalks, the terminals of which become free or proper leaves, as we commonly understand them. That this is the real truth can readily be perceived in many of the stronger growing Compositæ. In a *Centaurea macrocephala,* now before me, the stem can be readily perceived to be formed in this manner. The lower leaf in the spiral laps over the one next above it like a slate or shingle on a roof, and it is *the width in proportion to the length of the spiral coil that decides the numerical order in the phyllotaxis.* The acceleration or arrestation of the uncoiling energy will therefore easily explain for us these apparent aberrations from the normal condition. In some plants the energy born of the spiral motion does not cease with the unfolding of the membranous or leaf-blade portion of the structure. In many composites[6] there may be two or even three twists to the leaf-blade, giving it a singularly curled-up character.

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[5] *Centaurea macrocephala* is especially in mind here.

[6] *Senecio Doria* and *Centaurea macrocephala,* for illustration.

With these facts in mind the occasional facing of each other in leaves of *Lactuca Scariola* and other plants becomes explainable under the varying degree of energy expended in the uncoilings of the spiral at different times and localities in the growth of the parts.

An interesting fact in connection with inheritance presented itself in this investigation. As already noted, the plants from the solitary individual varied in numberless ways. The second year's crop had in the main the characters of the parent ; but there were groups here and there which, while varying among themselves, would have the general characteristics of the first variation. For instance, there would be a group of pinnatifid-leaved plants, though with variations in breadth and length, in the divisions of the lobes, and in other respects differing among themselves ; while in other spots would be groups with leaves wholly entire, and other groups with serratures. The seeds from some single plant, reaching the ground together in one place, probably accounted for this. It is at any rate a good illustration of inheritance going along in parallel lines with variation.

IX. THE STIGMA OF ASCLEPIAS.

While preparing the chapter on *Asclepias tuberosa* for my illustrations of the *Flowers and Ferns of the United States*, continued as *Meehans' Monthly*, I noted glandular callosities, not hitherto recorded as being observed, indicating abortive floral organs. It led me to a review of our former beliefs in regard to the structure of these curious flowers. The first morphological difficulty was the supposed five-lobed stigma. With our modern knowledge of morphology it seemed incredible that a flower formed on a quinary plan, losing finally by abortion three or four of its carpels when forming the fruit, should yet have a five-lobed stigma at the apex of its ovarium. A careful examination satisfies me that the accepted view is wrong. The mass forming the supposed stigma is wholly independent of the carpellary system. Indeed, there is no departure from the morphological laws prevailing in flowers generally. Keeping in mind the power of union and segregation, and of arrest and retardation in the development of floral organs, the structure of the flower in *Asclepias* will be found exceedingly simple. There is no occasion for the mystery brought in generally to account for a seeming departure in regular law, when accounting for its singular structure.

In tracing the morphological relationship of the parts of a flower, we are largely aided by examining a number of species. In this study of *Asclepias tuberosa, A. verticillata, A. Syriaca* and *A. incarnata* also took part.

From some vigorous shoots of *Asclepias incarnata,* the lower leaves showed that normally the leaves are alternate, and arranged on a $\frac{2}{5}$ plan. Whatever variation from this plan follows, must be referred to some discordant occurrences. In *A. incarnata,* we see how this operates in forming the branching character. The main stem is subtended, when it begins to branch, by a weak branchlet on one side and by a leaf without an axillary bud on the other. The exact fact is that what now appears the central axis or leading stem was the axillary bud! When the rhythmic growth-wave resumed activity the chief energy was directed toward the axillary bud. It pushed aside the former leader, and reduced it to a mere branchlet. We can still trace this disturbing force through many nodes, though with continued arrestations and developments it is often difficult to trace the originals eventually. But we learn by this to look for these aberrations of the various parts when considering *A. tuberosa* by itself. In the case of the latter species we find in the earlier states of growth the axillary bud is not able to dislodge the central shoot from leadership. In this case we have the branchlet in the axil of the leaf, and not opposite, as in the other species cited. Later on we may find the growth-energy directed strongly in the axillary direction. But we have learned the valuable lesson that variation in the period of rhythmic growth-energy is sufficient to produce specific characters.

With the ideal quinary plan in mind we find in *Asclepias tuberosa* five sepals, and the series of five petals is alternate with the sepals as such a series should be. Then we have, alternate with these, a series of five stamens, but the filaments have become united up to the connective (represented by a minute dorsal gland), what should have been anthers are petaloid, and we have to call that a nectary which should have been a more elaborate organ. The five perfect stamens next appear, and properly alternate with the five petaloid stamens (nectaries) below. Now we perceive what, if noted before, has not been recorded: five greenish scales, alternate with the filaments and connate with them, extending beyond the so-called stigmatic disc, and forming a crested covering for the disc at the same time. This series united with the lower portion of the filaments is also connate with the disc. These scales have formed so complete a union with the anthers, as to give the appearance of wings to these organs, but the tracing beyond the apex of the anther clearly indicates their original distinction. Coming now to the disc, we find that its lobes are alternate with the scales, and that they are opposite to the anthers. Terming all the various cycles of a flower independently of the carpellary or axial system, staminal or petaliferous whorls, the substance of the disc may reasonably be referred to a whorl of this character which has had its terminals bent over as the next outer whorl did, and unite to form a homogeneous succulent mass.

This is more than a conception. The alternate development of each cycle, just as we should expect them to be, brings the facts close to a demonstration. But an examination of the carpellary development makes the facts clearer. Taking a flower about to fade, remove the nectaries, make a vertical section just above the line of the petals, and then open the flower carefully, we find two separate and distinct styles imbedded into the succulent base of the disc. Though seemingly homogeneal with the disc mass, we can with care note that they really terminate at this point. Not only this, but we may *see the stigmatic points.* As the flower dries, the disc mass separates from the apex of the carpels, and falls with what we may surely call the next corollary system. In *Asclepias Syriaca,* the union of the real stigma with the supposed stigmatic disc has been so slight, that no sign of a cicatrice remains to mark the earlier connection.

The union of parts that we conceive to be normally separate is so characteristic of *Asclepias,* that we see the evidence in many ways. The breaking up of the quinary method in this way has already been noted. In *Asclepias tuberosa* this is further seen at the base of each umbel. Each flower, normally, is a primary branch, with the subtending leaf in the axil from which it sprang. But the bases of the flower branches with the subtending bracts have become united and succulent, with only here and there the point of a bract like the topmast of a sunken ship appearing just above the surface.

We may now conclude that there is no special mystery to be solved in the flower of *Asclepias.* The structure is very simple. The parts, as we conceive them primarily to be, are all there, and in their regular places. The tendency to arrestation and union accounts for what seems strange.

With the elucidation of the real structure of the flower, other mysteries will dissolve. No one has been able to explain the manner in which the flower is pollinized. The accepted hypothesis is the chance transference of a pollen mass to the disc by means of an insect's foot. The abundance and regularity of seed vessels on many species should at once discredit such a notion. Regularity does not follow accident. In almost every umbel in *Asclepias incarnata,* there are regularly one or two fertile flowers, and this is true of other species. As the anther cells rupture, the pollen mass is in close contact with what I have shown is the real stigmatic surface. The flower is an absolute self-fertilizer, and can receive no aid from the visits of insects.

The failure of so many flowers to mature fruit is a matter of nutrition. It has already been seen how easily the axillary bud is induced to displace the leader, by the diversion of nutrition in that direction. There are always two ovaries and corresponding styles in each flower of *Asclepias.* If we open a fertile flower in an early stage of its advance to a fertile condition, we may note that the stronger one has simply starved the other.

There are other matters in connection with the life-history of *Asclepias* well worthy of investigation. In *Asclepias tuberosa,* as in some other species, the petals reflex hurriedly when opening, but close in again when near maturity. The flowers that become fertilized coil as if they were tendrils, and indeed seem to prefer to twine around some object. One might almost expect to find some climbing species in the genus. It has been said that explanations of this character to be acceptable should be able to account for all phenomena of a similar character. In this case, what about the stigmas and general features of Apocynaceæ? Surely the staminal verticil has no relation to the disc-like character of the stigma in some members of the order, *Vinca,* for instance. But we do not know how far the consolidation of parts has gone. In a double-flowered oleander we note numberless petal-like organs that must have had a separate starting-point, and what appears as a single style is really made up of two, covered by a membrane evidently of staminal origin.

X. PHYLLOTAXIS IN CONNECTION WITH CHENOPODIACEÆ AND POLYGONACEÆ.

Phyllotaxis has not been made much use of by systematists. In a general way recognition has been given to the alternate or opposite arrangement of the foliage in connection with orders or genera. We do not expect to find alternate leaves in Rubiaceæ, and opposite leaves in Rosaceæ would be a surprise. But we may go further. In a general way we may look for a pentamerous arrangement of the floral organs, whenever we find a $\frac{2}{5}$ arrangement of the leaves. In Chenopodiaceæ and genuine allies, we find the $\frac{1}{3}$ plan prevail. We may doubt the relationship with Chenopodiaceæ of any plant with a $\frac{2}{5}$ arrangement.

In Polygonaceæ we find the $\frac{2}{5}$ arrangement the rule, and a close study of some species of the order shows that, though there are great deviations from a pentamerous structure, the pentamerous is the main plan, and the apparent deviations are the results of abortion, or of a union of parts usually separate.

Fagopyrum esculentum well illustrates this. The outer series which we take to be the calyx is pentasepalous. There are eight stamens, but if we examine the flower closely we find that there are only five in the verticil succeeding the calycine whorl, and these are alternate with the sepals as they should be. The next verticil appears to be of eight glands; but on close inspection there are but five, and these are alternate with the stamens. Then we have three rather smaller glands, just above the line of the larger ones, and making the eight which come under casual notice, and these three are at the base of the three stamens, which make the eight usually credited to the species. Here a puzzle presents itself. There should be two more glands to make a complete verticil of five, and two more stamens for another perfect verticil, and how does it come about that the stamen is just above the gland and not alternate therewith? We cannot answer; but with a clear case of three perfect verticils on the plan of five, we may assume that in some way, as yet unaccounted for, abortion and disarrangement of parts at this point have occurred.

The three upper stamens present an interesting feature that does not appear to have been hitherto noticed. The outer five have an incurved character, but the inner three curve outwardly, and the outer surface of the upper and lower anthers meet face to face. The pistils have an outward trend, or we may say recurve, as the three inner stamens do. I say pistils, for though it is customary to look on the flower as having but a single deeply cleft pistil, the three-valved ovarium clearly indicates a tricarpellary origin.

The fact that but a single seed is found in the capsule must be attributed to abortion, just as we find but a single seed in many pluri-carpellary rosaceous fruits; and to the same principle of abortion that has already operated in breaking up the pentamerous character on which, as we have seen, the flower is normally planned. Instead of a relationship to Chenopodiaceæ, as usually suspected, or with Caryophyllaceæ, as has been suggested, its proper position is evidently nearer Rosaceæ than with either. In *Spiræa,* for instance, say *Spiræa opulifolia* L., we have the persistent five-cleft calyx, but only three carpels in many instances, with remarkable irregularity in the number of the seeds. Then, proceeding to *Neviusia,* we have the permanent calyx doing service for petals just as we find in *Fagopyrum,* and we have a little disc on which are the stamens, corresponding to the glands on which are the *Fagopyrum* stamens. Further, we have the ovaries reduced to two, with a single ovule.

This conception of the relationship of Polygonaceæ to the Rosaceæ enables us to account for the stipules, characteristic of Rosaceæ, but which are wanting in Chenopods. That the ochrea in Polygonaceæ is but a stipule which may become united so as to clasp the stem is universally conceded.

A feature that has been overlooked in *Fagopyrum* is particularly instructive. Even the most recent descriptive work, Britton and Brown's *Illustrated Flora of the Northern States and Canada,* says of it, " glabrous except at the nodes." But one-half the stem and its inner face are hairy, only the exterior half is glabrous. It has been wholly my work to show that leaves do not originate at the node from which the leaf-blade springs. I contend that the whole of axis or stem is made up of the sheathing bases of the leaves, the blade being simply the departure of the upper portion, when the cohesive power has been reduced in force. The margin of the leaf-blade in *Fagopyrum* is ciliate. The line of hair on the stems of the plant are simply the strongly developed cilia on the united edges of the clasping leaf-bases.

An interesting circumstance in connection with *Fagopyrum* is the tendency to sterility of the flowers under certain conditions. Farmers know that if buckwheat is sown early in the season, the seed crop is light. Examining plants on the 22d of July, that had come up from self-sown seed in early spring, out of many hundreds of flowers, only one had perfected a seed. The anthers were destitute of pollen. This may have been the result of unusual vegetative vigor, which vigor has to be somewhat checked in most plants before fruitfulness is favored. The plants examined were growing in very rich soil, and unusually luxuriant; but the fact shows how much conditions have to do with sexual affairs.

XI. THE INFLUENCE OF FUNGI ON THE FORMS AND CHARACTERS OF PLANTS.

The influence of microscopic fungi in changing the form and character of vegetation is well-known in connection with monstrous conditions. It does not, however, seem to have occurred to biologists that what occurs in an exaggerated degree may reasonably be expected to prevail as a rule. The power that can induce vegetation to run into monstrosities may be so able to control its forces as to be a regular factor among the laws of form.

I believe that Prof. Farlow and some German botanists have recorded cases where the action of minute fungi has changed the character of plants to a degree that might be almost termed specific. I have myself, in a communication to the Academy of Natural Sciences of Philadelphia (see *Proceedings,* 1874, p. 146) shown that where *Euphorbia maculata* is attacked by an *Æcidium,* it assumes characters very little different from *Euphorbia hypericifolia.* Instead of trailing, the plant becomes erect. The hairy stem and fruit become perfectly smooth, and the swelling at the nodes that characterizes the latter species is assumed under the changed conditions. Only for the evidence furnished by the reproductive stage of the fungus, a botanist might well be pardoned for referring the attacked specimens to *Euphorbia hypericifolia.* Assuredly, *E. maculata* would not be considered in the determination.

Recently a specimen of a singular combination was placed in my hands by Mr. W. Finger, of Milwaukee, Wis. A pair of long-stalked, smooth and shining leaves seemed to proceed from a root stock of *Hepatica acutiloba* D. C. They were strikingly suggestive of some undescribed species of *Ranunculus.* The collector wás suspicious that some other plant had managed to force its leaves through the root stock of *Hepatica,* as instances of this behavior in some plants have been known. Though nothing pointing to this could be ascertained, the specimens were sent to me under an impression that there must have been some such occurrence. The leaf-blades, however, showed, by the slightly developed fructification, that the whole character of the leaf had been changed through the medium of a minute fungus, apparently closely related to an *Œcidium,* as had been found connected with the *Euphorbia.*

In the paper on *Euphorbia* cited, I ventured the proposition, as deduced from the observations recorded, that "certain phases of nutrition brought about by the attack of a fungus may change specific characters." The experience here related not only affirms this proposition, but indicates that the changes induced follow definite lines. In the *Euphorbia* the result of the operation of the fungus was especially marked by the elongation of the nodes and the total suppression of the hair that densely clothes the species attacked. In this case we have again the hairy character of *Hepatica* totally obliterated, and the petiole elongated in a striking manner.

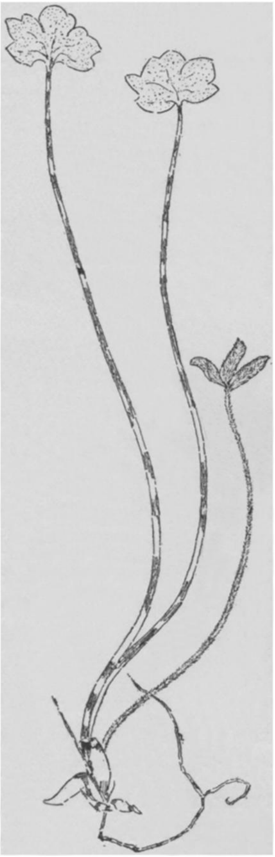

FIG. 1.

In view of the modern discoveries of symbiotic relations between algæ and fungi in the development of lichens, and of species of fungi in connection with the life-histories of other plants, there can be no extravagance in the suggestion that in some manner as yet undiscovered these minute organisms may play an important part in the origin of form. In this case nothing but the advance toward fructification would have furnished ground for suspicion that the leaf was not in a normal condition.

XII. THE MOVEMENTS OF PLANTS.

What is known as Heliotropism has never been clearly understood. Much that has been referred to the action of the sun may be attributable to other causes. When Moore sang:

> "As the sunflower turns on her God, when he sets,
> The same look which she turned when he rose,"

he expressed the prevailing thought that the plant, in an active sense, sought the sun or avoided the sun, as the case might be, and not that the sun was the acting direct agent in drawing the flower unwillingly along.

On August 20, 1898, I was passing a large mass of *Helianthus orgyalis* at about sundown, and noted that they were taking a wide curve toward the northeast, the stronger plants making a curve of nearly a foot in diameter. As the thermometer had been ranging between 80° and 90° Fahr., I passed on with a general impression that excessive transpiration had induced wilting. On the evening of the 25th, observing again this general northeast curve, I attempted to lift one of the curved points, and was surprised to find the stem at that point rigid, and not flaccid, as it would have been if suffering from lack of moisture. Careful and continued observation was decided on.

On the 26th, at 6.30 P.M., the stalks which at sundown the previous evening were curving northeast were now erect. On the 27th, at 6 P.M., the stalks were curving again northeastwardly. On the 28th, at 11.30 A.M., all were curving slightly southwest, and by 2 P.M. the same day were nearly erect. At 6 P.M., with their usual curve northeast.

On the 31st, at 6.30 A.M., found the first deviation from that general plan in having the curves northeastwardly, instead of having erect stems. At noon they were erect, except a few of the weaker ones which had a northwestern curve. It should be here noted that the terminal flower, which in this and many other composite plants is the first to open, while the side branches rapidly develop, was now expanding, but many of the stronger branchlets joined with the weaker main stems in this northwestern curve. Even where the main stems by some accident had a southern or eastern leaning, the branchlets had the northwestern curve. Observations were continued till the evening of September 4, all resulting in the fact that the general rule in this species was that the curving was at nightfall northeastwardly; at 6.30 A.M., erect; at noon, with a slight curve, and at 6 P.M., a strong curve northeastwardly.

On September 6, *Helianthus Maximilianis* had developed considerably toward a flowering state. At 7 P.M., all the flower stems were bending slightly westwardly. On the 7th, at 7 A.M., all trending eastwardly; at 6 P.M., westwardly. On the 8th, 9th and 10th, the observations were the same. There was no opportunity to note the direction of the stems at midday, but on the 11th they were nearly erect, but with a very slight eastern trend. At 5 P.M., quite erect. By this time the ray florets of the terminal head of flowers were expanding, and no curving was noted subsequently.

In these observations I thought to note whether the curving was due to an uncoiling of the spiral growth, or was done by a direct lifting upward and over to the opposite point of the compass. Pins, with their heads pointing east, were inserted in the stems just beneath the terminal flower head. Through all the variation in the direction of the curves, the heads remained unchanged in direction. This proves it was not an uncoiling operation, but done as a purely ascending and descending act.

Further, these observations show that the curvature proceeds at night to the same extent as by day. Light, therefore, or the attraction of the sun's rays, must be eliminated from the factors in the work, especially as the evening curvature of one species is in a different direction from the other. So far as these observations go, the cause of motion is still obscure.

While these observations were in progress, I noted on the evening of September 5 that a large plant of *Palafoxia linearis* that certainly had all its branchlets erect in the morning were beautifully curved at 6 P.M., but they were in no particular direction of the compass. The curved branches were so rigid that they resisted considerable force in the endeavor to straighten them. The next day at 7 A.M. they were perfectly straight. The observations were made daily till the 11th, with the same results, the only additional fact being that the curving seemed to commence about 3 P.M., and to reach its maximum about 5 P.M. The stronger branches had the greatest curve, in some cases making a complete circle, the point of the branchlet reaching the main stem, the curve of the weaker branches usually reaching a half circle. On the evening of the 7th I tied a light label weighing an eighth of an ounce to the apex of one strongly curving branchlet. It had no result in weighing the branchlet further down. In the morning it still held the branch down to the point I had left it the evening before, though the point beyond the string had grown a little and taken an upward curve. When I cut the string with the point of a scissors, the branch flew upwards with a sudden spring, reducing the curve from a half to the quarter of a circle. The stem retained this light curve during the whole of its subsequent growth. In this connection it may be noted that in the reflex from the curve to erection in *Helianthus orgyalis*, the stem never reached a perfectly straight line, and hence the stems of this species are *generally flexuose.* That the curves in *Palafoxia* should be in any direction of the compass indifferently, and yet in specified directions only in the two Helianths, is remarkable.

While taking notes on these plants at dusk one evening, I noted that a plant of *Helenium autumnale*, with hundreds of unopened flowers, seemed drooping for want of water. Calling the fact to the attention of Mr. Hemming, in charge of my hardy herbaceous department, he felt sure that it only needed water, and this was copiously applied, with no result. Closer examination showed that the common peduncle was bent in the middle, the upper portion forming a right angle with the stem. So rigid was the bend that they could not be straightened without injury. The angle is maintained for a number of days till the ray florets become visible, when the peduncle gradually straightens, taking several days to complete the task. As in the case of the Helianths, pins were inserted in order to note any uncoiling process if present. The straightening was done by a purely ascending process.

The gain to science by these observations is negative. It is clearly seen that what we have understood as heliotropism in connection with plant movements has no place in the phenomena. It may be assumed that we have to look to various phases of life-energy in the plants themselves for the final explanation.

XIII. ECCENTRICITY OF THE ANNUAL WOOD CIRCLES IN RHUS TOXICODENDRON L.

In an abstract of an address[7] made by myself on excrescences and eccentric wood growth in trees, I pointed out that the thickening of the annual layer of wood, more in some portion of the circle than in others, was due to a greater generative power of the cells in the thickened portions owing to more abundant nutrition having reached them. "Where the whole mass of tissue seems equally and regularly vitalized, the generative tissue forms a new layer of wood of about equal thickness all round." "In some cases masses of cells seem to easily draw from others more than their share (of nutrition), and the latter are correspondingly weakened." "All the cases of peculiar eccentricities, *Hedera*, *Toxicodendron*, *Ampelopsis*, and the peculiar cases in ordinary timber trees, could be explained by this, so far as to note that the immediate law was a loss of generative power in the cells of the annual layer. Of course the indirect causes leading to this would be numerous, and left room for much more investigation." At the conclusion of that address the report published says Mr. Meehan was asked to explain the square section of trunk of *Picea amabilis* on exhibition at the American Centennial, and the reply was that in all probability the thickened portions of the annual layers at the corners forming the square were due to strong roots near these four corners supplying abundant nutrition in those directions.

Though twenty-two years have elapsed since that communication was made, the matter does not seem to have been further investigated, and our biological text-books still endeavor to explain these variations in the width of the concentric rings to various causes other than the one to which I have alluded. Though the bulk of nutrition must be by way of active roots, the carbon necessary for the structure must be contributed through the leaves. Healthy leaves and vigorous roots must be reciprocal. On the other hand, the power exercised by any force is in inverse ratio to distance; and it is probable that a weak series of foliage, near the active roots, might aid nutrition to better advantage than stronger ones further away.

Some interesting observations were recently made on the ordinary poison vine, *Rhus Toxicodendron* L. A large stem that had for a quarter of a century been attached to an old cherry tree was cut down. From this were many old branches, which had not attached themselves to anything, and had no rootlets, as those attached to the tree had. In none of these latter cases was the wood eccentric, while even quite young branches, rooting to the bark of the cherry tree, were eccentric. Fig. 3 shows a section of the main stem, cut across two feet from the ground, showing the attachment by its numerous fibrous rootlets to the old bark at the back. Fig. 2 is a section of a seven-year-old branch which has not thrown out rootlets. From an examination of a number of stems with and without rootlets, the fact was clear that the rootlets decided the eccentricity. They evidently introduced nutritious matter from the old bark to which they clung, and the cells nearest to them derived the chief advantage. The concentric rings would inevitably weaken in proportion to the distance from the source of supply. If the woody circles had been all of equal thickness the pith would have been in the centre, as we see in Fig. 2.

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[7] Proc. Acad. Nat. Sci., December 19, 1876.

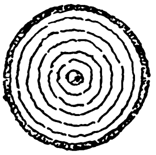
FIG. 2.

In the contribution reported in the *Proceedings* as above cited, the weakened condition of the wood cells, and their overgrowth by stronger ones is given to account for the bark often found inside the trunks of *Bauhinia*, *Wistaria*, and other woody plants. We see in Fig. 3 how this can easily be brought about. If some good roots in connection with healthy leaves should appear in connection with the lobes represented by the heart-shaped figure so as to reinvigorate the depauperate wood cells, the bark would soon meet, and eventually be wholly enclosed.

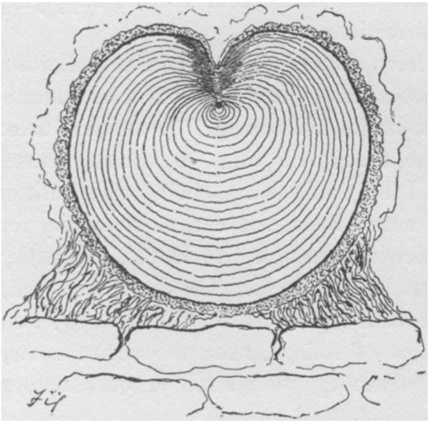
FIG. 3.

It may be added that the fibrous roots of the Poison Vine are all annual, but they become very wiry after death, and continue apparently for all time to aid in sustaining the plant's attachment to its support until violently separated by external forces.

XIV. Morphology of the Grape.

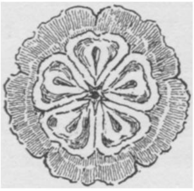

Fig. 4.

In referring to the grape or other vitaceous plants, it is customary to say of the tendril that it is a reduced branch. It was long a puzzle to me that a branch should appear on the opposite side of a leaf, which leaf should have no axillary bud, and, further, it seemed remarkable that when there was an axillary bud at the base of the leaf, there was no tendril on the opposite side.

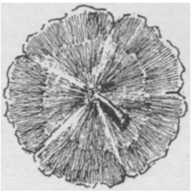

Fig. 5.

I have since learned that in these cases the plant simply presents to us phenomena very common in plants, especially when forming an inflorescence, of displacing the leading stem, and replacing the leader by the growth of the axillary bud. What was the leader is pushed on one side, and seems to be, what writers on the vine assume the tendril to be, a mere branch. In vigorous branches of the grape vine, there seems occasionally to be an axillary bud opposite the tendril, but this is simply a supra-axillary bud, the upper of the two having advanced to the position of a leading stem, when the former leader had been reduced to the condition of a tendril.

The fact is of importance in tracing the genital relationship of the order of grape vines with other families. There is much in Vitaceæ to suggest an alliance with rhamnaceous plants. In Celastraceæ, for instance, the character noted in the grape of having the tendril opposite a leaf without an axillary bud is wanting simply owing to the opposite-leaved method of carrying its foliage. How the growth-energy has been diverted from the leading shoot to the axillary buds is shown in the inflorescence of *Euonymus Japonicus*. With a further suppression of the leader there would have been dichotomous branching. With an alternate system of leafage, the central axis would have been pushed aside to become a tendril or some other lateral organ. The stronger shoot from the axillary bud would then be the leader. In many rhamnaceous plants, notably in some species of *Zizyphus,* though the axillary bud has sufficient force to occupy a position erect and in a direct line with the branch that bore it, thus pushing aside the former leader, it has not energy enough to maintain the leadership. The main branch, though forced from the direct line, continues to lead, and then we have the zigzag or flexuose stem.

Another evidence of the close relationship of Vitaceæ with the rhamnaceous family is furnished by an occasional tendency in grape berries to assume a valvate capsular form. A case of this some kind fell under the notice of Fl. Von Schlechtendal, who thought it of varietal importance. He figures and describes it in the volume of *Linnæa* for 1830 as *Vitis vinifera capsularis.* Morphology had not reached the importance it has arrived at to-day. My first opportunity to examine a case of the kind was furnished by Dr. James A. Darrach, of Germantown, Philadelphia, who found it on a bunch of the Flame-colored Tokay, common on our fruit stands as the California grape. It is capsular, five-parted, disclosing the naked seeds attached to an axile placenta, the whole very suggestive of some celastraceous plant.

The grape is notorious for other morphological vagaries, among which the total absence of seeds in some varieties is to the point. The Corinth, Mokunha and Sultana are also cases. These furnish the currants of commerce. Seedless berries are common among the fertile ones in many varieties of American species. In one known in gardens as the Lindley, one-half of the berries will be seedless, and sometimes the whole bunch will be of that character. In these the berries do not reach more than one-half the size of the seed-bearing ones. The manner in which this variation is accomplished has been ably set forth in the *London Journal of Horticulture,* of September 3, 1896. It is well-known that many fruits will advance considerably in size in the absence of pollination. The fruits are seedless, and they never reach the dimensions of the fertilized fruits. The osage orange and the cucumber are familiar instances. In the growth of cucumbers under glass every female flower produces a cucumber, but if the grower desires large and fine fruit, he uses the male flowers directly on the females. The isolated female osage orange has the seedless " balls " but half the size of the fertile ones. In the paper on the grape above cited, the author traced the successive stages of growth in the fruit by the aid of a powerful microscope. The mother or berry-bearing plant has the power of growth up to a certain stage. Ovarium, ovules and every part advances up to about half the size of the full-grown stage, when it awaits the entrance of the pollen tube. If this tube fails to appear at this stage, the funiculus or neck connecting the ovule with the placenta dies, and this ends all future growth. The grape is a plant having female flowers, male flowers and neuters. It is among the latter that the currants or seedless grapes appear. The ovaries and ovules are perfectly formed, but the stigma is imperfect and will not admit the entrance of the pollen tubes.